guage is, as he contends, "the only basis on which the subdivision of the race should proceed," is a question which needs rather fuller discussion than Dr. Brinton has devoted to it. It may, however, be admitted that in the study of the American aborigines, at the present stage of ethnographical science, no other test works so well upon the whole as the linguistic; and that the results attained by the use of it are interesting and instructive, whether they exactly correspond to racial distinctions, in the strict sense of the expression, or not. Dr. Brinton divides the American race into five great groups—the North Atlantic group, the North Pacific group, the Central group, the South Pacific group, and the South Atlantic group. Taking each of these in turn, he describes the various stocks included in them, paying attention especially to those portions of the continent about which ethnographers are least united. The facts are sometimes presented in a style unnecessarily dry; but the author has taken immense pains to arrange them clearly, and the work will be of genuine value to all who wish to know the substance of what has been found out about the indigenous Americans. We may note that Dr. Brinton does not agree with those who hold that the Red Men originated in America. On the other hand, he sees no reason to suppose that they came from Asia. His theory is, that they were an offshoot from the race which in the earliest times occupied Western Europe, and that they crossed from the one region to the other on a land-bridge. The former existence of this land-bridge he accepts chiefly on the evidence collected by various English geologists.

Charts of the Constellations. By Arthur Cottam, F.R.A.S. (London: Edward Stanford, 1891.)

THE large edition of Mr. Cottam's magnificent charts has met with a well-deserved success, and the smaller edition before us will doubtless be received in a similarly favourable manner. The thirty-six charts in the large edition measure 30 × 22 inches, and were projected on the scale of one-third of an inch to a degree of a great circle; the size in the reduced form is 15 × 12 inches, and the scale is one-half as great. In this handy size the charts will be sure to have a wide circulation among beginners in the study of astronomical science, and to possessors of the smallest telescope with alt-azimuth mounting they are invaluable. The road to an intimate knowledge of the stellar universe is considerably straightened by showing each constellation on a separate chart, whilst three key maps exhibit the relative positions of the various constellations, and will be extremely useful to show alignments over larger spaces than are included on the separate charts. Some notes on the constellations, in conjunction with an excellent introduction explanatory of various astronomical measurements and methods, enhance the value of the charts and help to render the work superior to any hitherto published. G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Co-adaptation.

THE remarks on this subject made by Dr. Romanes (NATURE, March 26, p. 489) do not appear to me to present the case in a fair light, and I therefore think it advisable to make some further observations. In the first place, I do not allow that there is any "fortuity" in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual, for every variation, of whatever kind, must of necessity entail numerous structural and functional modifications. If variations in the length of neck or leg or wing, in the strength of certain muscles, or even in colour, arise "fortuitously" (meaning thereby through some unknown cause antecedent to birth), there must be corresponding variations in the length of the bones, nerves, blood-vessels, &c., as well as in the physiological processes upon which depends the supply of nutriment to the parts in question. If any variation survives, or is perpetuated by breeding, it must therefore possess not only the external visible character A, but also all the concomitant and less obvious or altogether undiscoverable modifications, B, C, D, &c. However complex the correlated modifications may be, the mere existence of the variation implies the existence of such modifications, and the survival of the variation implies the survival of these correlated modifications. If, therefore, it is argued that any variation is a simple case of modification in some one character only—as might be inferred from Dr. Romanes's letter—this is an unwarranted assumption entirely opposed to fact.

But the case we have to consider is that in which the variations A, B, C, D, &c., are not physically or physiologically correlated, but in which they are supposed to be "independent variables." It is to such cases that Mr. Spencer applied his objection. He says ("Factors of Organic Evolution," p. 14):—"What shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?" Then comes the case of the giraffe, in which Mr. Spencer endeavours to show that the fore- and hind-quarters of this animal must be regarded—if I may venture to express his views briefly—as, "independent variables."

In reply to arguments of this kind, it may be pointed out that mere remoteness does not preclude the possibility of there being correlated variability between structures or functions. And in the next place even if it be admitted that certain parts now co-operative were independently variable at first—as in many cases they no doubt were—it is a gratuitous assumption to suppose that they must have originated from the very beginning in individuals in which the parts were co-operative, *i.e.* in individuals in which the non-correlated variations, "A, B, C, D, &c.," occurred simultaneously. The chances against such variations occurring in any one individual are, I concede most fully, "infinity to one." But if A (with its correlated physical and physiological modifications) survives through natural selection (utility) or is bred by artificial selection, that form is fixed by virtue of its possessing certain characters. If it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*. Thus with the giraffe it must be shown that the heightening of the fore-quarters not only conferred no particular advantage, but may even have placed the animal at a disadvantage unless associated with the necessary modifications of the hind-quarters, the necessary modifications being, according to Mr. Spencer, those required to enable the animal to escape pursuit, "since any failure in the adjustment of their respective strengths [the fore- and hind-quarters] would entail some defect in speed and consequent loss of life when chased." To me—and I speak with all deference to the views of Mr. Spencer—this seems like an imposition on Nature of certain conditions which might not be true. It cannot be dogmatically laid down that at the time of its development from the ancestral form the necessity for escaping pursuit was of equal or greater importance than the faculty of high-reaching. In fact, the heightening of the fore-quarters is just as likely to have *diminished* the necessity for great speed, by enabling the animal to take a wider survey of its surroundings, and so to get sight of its enemies in time to escape. But, if speed were essential for the preservation of the species, I must confess that I see no difficulty in the belief that this character also might be superadded subsequently to the heightened fore-quarters in the same way that other associations of characters arising from "independent variables" are fixed by artificial selection. I do not admit therefore that "all this is quite wide of the mark"; on the contrary, I believe that it is *the* mark, and that the particular case of co-adaptation quoted by Mr. Pascoe (I am afraid in a somewhat garbled form) may resolve itself into a "confluence of adaptations." In fact, I do not think that it

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would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race.

The remarks with which I have endeavoured to meet the arguments advanced by Dr. Romanes ignore the "difficulty" which he have interposed with respect to the want of analogy between artificial and natural selection. I have left this out of consideration advisedly, because it raises one of the questions which have so long divided Dr. Romanes from those whom he has thought proper to label "neo-Darwinians." It is the old difficulty of "the swamping effects of intercrossing" under another form. I do not believe in the reality of this difficulty, and it has been so amply treated of by Dr. Wallace and others that I do not feel warranted in trespassing upon these columns at any greater length in order to rediscuss the question. I can only assure those who have read the comments made upon my review of Mr. Pascoe's book that I have not been made the subject of any metaphorical fraud, but that I accept the analogy between artificial and natural selection as real.

R. Meldola.

The Meaning of Algebraic Symbols in Applied Mathematics.

Dr. Lodge's remarks on p. 513 (April 2), ought not, I think, to pass without protest. He very reasonably objects to being asked to use a formula which is adapted to one particular set of units, and is not convenient for any other set, and prefers the greater freedom which is usually indulged in, as regards units, in mathematical physics. But he goes further than this, and maintains that it is best (he almost suggests that it is necessary) that Prof. Greenhill's practical man, if he wishes to avoid the somewhat mild difficulties which at present beset him, should adopt the system set forth in Nature, vol. xxxviii. p. 281. It may be a reasonable thing to do to base the interpretation of physical equations upon the method of multiplication of concrete quantities described in the article referred to; but that the practical man, whose difficulties are in question, is likely to take the trouble to understand it, may be confidently denied.

However, I think that the system of interpretation advocated involves other inconveniences. Suppose, for some purpose, we chose to measure the velocity of light by the distance of the light of that colour from a given line in a standard spectrum, thus giving velocity for this purpose the dimension length. This quantity would have different properties from the same velocity measured in the usual way; it would practically be a different quantity; yet the velocity of light is independent of the mode adopted for measuring it. Does it not make confusion to insist upon one of these two quantities being the concrete velocity itself, some other name having to be invented for the other? The same confusion of language would arise even if we desired to depart so slightly from the usual practice as to use our velocity symbol for the specification of it in miles per second, using a centimetre for the unit of length.

I have found that, occasionally, a good way of clearing up difficulties of pupils about dimensions is to say that it is no more intrinsically absurd to equate an area to a length than to equate a length to a product of a velocity and a time, all the symbols being numbers; but that the latter equation can be employed without abandoning the particular freedom as to subsequent choice of units which we wish to retain, and that the former cannot. I have heard a lecturer appeal, in a similar case, to the intrinsic absurdity of saying that the area of a field is equal to the distance from here to London: this appears to me to be not so clear a way of talking of the distinction between the two cases.

Finally, what are the disadvantages of considering the symbols of quantity to be mere numbers? Dr. Lodge, while making out his case as against Prof. Greenhill, is, to my mind, quite unconvincing on this more general point.

King's College, Cambridge, W. H. Macaulay.
April 11.

Force and Determinism.

Dr. Oliver J. Lodge has reminded us, in your issue of March 26 (p. 491), that under physical constraint or control the direction of motion of material particles may be changed without expenditure of energy or performance of work.

It does not follow from this that the direction of motion can be changed under metaphysical control or constraint—that is to say, by vital force, mind, will-power.

If mind controls the motion of molecules, then mind must be reckoned among the physical forces, differing, however, from all other modes of physical force in that it always acts at right angles to motion.

It is right that your readers should be informed that there are many philosophers (I allude especially to those who accept the hypothesis of scientific monism) who regard the supposed control of mind over matter, or of matter over mind, as an assumption which is unnecessary and unsatisfactory.

An arbitrary alteration of the weather might involve no contradiction of the principle of the conservation of energy, and yet at the same time completely upset our notions of physical causation by the introduction of metaphysical interference.

If the distinction between generating motion and directing motion is one useful to remember, that betwen physical and metaphysical control is still more important.

University College, Bristol. C. Lloyd Morgan.

The Influence of Temperature on the Vagus.

In Nature of April 9 (p. 548) I notice that Dr. G. N. Stewart says: "nor has the influence of temperature on the vagus been before studied by a suitable graphic method." It appears to me that this sweeping assertion is not in accordance with facts, for in my paper upon the "Influence of Temperature on the Pulsations of the Mammalian Heart and on the Action of the Vagus," published in the St. Bartholomew's Hospital Reports for 1871, p. 216, I described a graphic method invented by Prof. Stricker, of Vienna, and employed by me with very satisfactory results. The apparatus may have been ruder than that employed by Dr. Stewart, but the tracings obtained by it were sharp and clear, and allowed the beats of the heart to be easily counted, even when the pulse rate was 470 per minute. The important fact that the peripheral ends of the vagus are not paralyzed by heat, but retain their power to the last, was clearly demonstrated in this paper. T. Lauder Brunton.

Antipathy of Birds for Colour.

At this season the yellow crocus is coming up, and again being destroyed, either playfully or maliciously, by the house sparrow. Has the bird an aversion to yellow? This could be tested by throwing bits of coloured wool about, and finding what colours are used in nesting. I should be glad to learn if this has been tried, and the result. M. H. M.

April 13.

The Discovery of Comet *a* 1891.

It is stated that Prof. Barnard, of the Lick Observatory, Mount Hamilton, anticipated me in the discovery of comet *a* 1891, as he first saw the comet on the evening of March 29, whereas my observation was made on March 30. My information on the point is somewhat meagre and uncertain, but it is very likely to prove correct. If so, I must relinquish my claim to priority, and comfort myself with the reflection that my discovery was an independent one, and that it conveyed the first notification of the comet received by the chief Observatories of Europe.

The *Times* of April 1 published a telegraphic message, dated Mount Hamilton, March 31, as follows:

"A small but fairly bright comet, with a tail of 15 minutes in length, was discovered by Prof. Barnard at 8.34 on Tuesday night at the Lick Observatory, the position being right ascension one hour ten minutes and ten seconds, north declination 44 degrees 48 minutes, moving rapidly southwards in the direction of the sun at the rate of one degree per day."

Now this telegram, if correct, would show that Prof. Barnard's discovery followed, not preceded, my own (made at 9h. on Monday night, March 30), and several scientific journals have alluded to the matter on the basis of the paragraph above quoted. But I think it highly probable the *Times'* telegram contained several inaccuracies, and that Prof. Barnard first saw the comet on Sunday night, March 29. Further particulars from America will no doubt shortly come to hand. If Prof. Barnard is entitled to priority, let it be freely accorded to him, and he will have my sincere congratulations on another success in a field where he has previously earned such high distinction.

Bristol, April 11. W. F. Denning.

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